Dietary carbohydrate restriction: Compelling theory for further research

To the Editor:

We commend the authors of this study for promoting a novel approach to type 2 diabetes management [1]. The physiologic argument for carbohydrate restriction in individuals with insulin resistance or deficiencies in insulin secretion is very compelling. However, as compelling as their argument may be, we recommend exerting caution and exercising due diligence before recommending changes to treatment guidelines.

The authors criticized current dietary recommendations for diabetes management, saying that these recommendations were developed during a trend of recommending low-fat diets. Low-fat diets were recommended primarily based on compelling physiologic arguments—but more recent evidence from randomized controlled trials (RCTs) has shown that dietary fat, except for trans-fat, is not as dangerous to health as was once thought [2,3].

The authors make the assertion that their conclusion is "sufficiently compelling that they feel the burden of proof rests with those who are opposed." In support of their conclusion, the authors wrote a narrative review that uses an interesting array of sources as references—including a few systematic reviews and RCTs, but also observational studies, pilot studies, uncontrolled trials, online discussion groups, and popular diet books. We would consider this nonsystematic review incomplete: There were a number of systematic reviews and Cochrane reviews on the topic not included in their article [4–9]. We would argue that there are gaps in the knowledge needed before recommending carbohydrate restriction, including the extent of restriction required to see benefits—as the authors themselves laid out, there are multiple definitions of carbohydrate restriction.

Instead of putting the onus on the opposition to design and conduct trials that could very well change type 2 diabetes management, we would take a more cautious approach to say that they have sufficiently compelling information for a hypothesis that would make very interesting RCTs.

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Carbohydrates for people with diabetes is not cautious

I thank the Drs. Fenton for comments on our review [1]. Our point, however, was that in diabetes carbohydrate restriction *is* a cautious approach. The Fentons provided no evidence that it is not. Science does not start from scratch. Diabetes is a disease of carbohydrate intolerance. It is intuitive that carbohydrate restriction is the first thing to try. It is not novel: Hamdy equated the original Joslin diet with the Atkins diet [2]. Some form of carbohydrate restriction is, in fact, widely used in clinical practice. We are counseling a systematic approach. Most of the authors of our review have extensive experience treating patients with low-carbohydrate diets with clinical successes over dozens of years, totaling thousands of patients. Conversely, recommending significant amounts of carbohydrate for people with diabetes, knowing that it will increase blood sugar, increase triacylglycerols, lower high-density lipoprotein, and increase the need for drugs seems to me to be reckless.

That "low-fat diets were recommended primarily based on compelling physiologic arguments" is not tenable. Numerous scientific papers, including those cited by the Fentons, and many scientific and popular books, including my own [3], show that low-fat ideas have been based on very poor science.

Given our limited success with current treatment guidelines, due diligence is required before *continuing* these guidelines. I do not know of any randomized controlled trial (RCT) that contradicts the evidence that we presented. What standard of success are we supposed to meet?

Our review, although narrative in form, *is* a systematic review. Our system was to search the literature for papers that, regardless of arbitrary classification, bring out important points. We sought well-designed studies that provided clear conclusions. Levels of evidence, gold standards, and other arbitrary classifications are unknown in other sciences; the best experiment is the one that answers the question at hand. As studies become longer and include more people, control of key variables is reduced and outcomes generally become less certain. Different approaches make a contribution but many systematic reviews, including Cochrane, take studies at face value, do not analyze experimental design, show slavish dependence on statistics and consistently confuse statistical significance with clinical or scientific importance.

An RCT might be valuable, but those who have been funded for such trials have had the opportunity to include a low-carb arm.

That they have not done so, as in TV courtroom dramas, goes to credibility.

Perhaps what is really bothering the Fentons, as it bothers my medical students, is the idea that a large part of the medical establishment has gotten things very wrong. I ask my students: "Do you think that there has ever been a period in the history of medicine where the great majority of physicians and scientists held to views that were not only wrong but dangerous and refused to change in the face of contradictory evidence? Do you think that there has ever been such a time? If you think so, you must at least consider the possibility that this is another such time."

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Re. "Early oral refeeding based on hunger in moderate and severe acute pancreatitis: A prospective controlled, randomized clinical trial": Can we really do without enteral nutrition?

To the Editor:

The recent *Nutrition* article by XL Zhao et al. entitled "Early oral refeeding based on hunger in moderate and severe acute pancreatitis: A prospective controlled, randomized clinical trial" addressed a very relevant question [1]. Even if the concept of pancreatic rest has been abandoned in recent years, the feasibility of early oral refeeding in cases of severe pancreatitis remains a very pertinent issue. Nevertheless, we believe that the authors were not able to establish conclusively the superiority of early oral refeeding considering the design of the study. First, the study was inadequate in mixing moderate and severe pancreatitis because the feasibility and the efficacy of oral refeeding in moderate acute pancreatitis is well established [2]. Severe pancreatitis represented only 27% of the patients in this study. On the other hand, the control arm did not correspond to the gold standard treatment recommended in international guidelines [3]. Ninety-seven percent of patients were treated by parenteral nutrition, even though enteral nutrition had previously proven superior. Two meta-analyses showed that enteral nutrition decreases infectious and surgical complications and mortality in comparison to parenteral nutrition [4,5]. Moreover, the patients in the study received other treatments (antibiotic prophylaxis, somatostatin analogs), which are not recommended [3]. Another major limitation of the study was that oral refeeding was not really early. The patients fasted for several days. The authors provided no information about proteins and calories actually ingested, but the duration of fasting after the onset of abdominal pain was 8.3 ± 3.9 d in the "early" group and 10.5 ± 5.1 d in the conventional group. Such delayed feeding could be deleterious considering the proven efficiency of early nutrition in acute pancreatitis to maintain gut barrier permeability and to prevent bacterial translocation [6,7]. The major point demonstrated by this study was that the patients were not hungry during the acute phase of pancreatitis, thus enteral nutrition should have been initiated as soon as possible (preferably in the first 24–48 h) after pancreatitis onset. Other studies are needed to be able to reach a conclusion about safety and feasibility of early oral refeeding in severe acute pancreatitis. Considering the frequency of acute pancreatitis in the world, this issue remains a major challenge.

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"Re. 'Early oral refeeding based on hunger in moderate and severe acute pancreatitis: A prospective controlled, randomized clinical trial.' Can we really do without enteral nutrition?" Author's response

To the Editor:

Included in this issue of *Nutrition* is a letter by Dr. Benoît Dupont regarding our recently published paper, "Early oral